PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

March 21, 1874.

I. *On the new Contact Theory of the Galvanic Cell.*

By J. A. Fleming, *B.Sc., F.C.S.*

The contest that has for so long been waged between the supporters of the two theories of the galvanic cell, the *contact* and the *chemical*, can hardly be said to have been brought even now to a decisive issue. For although the contact theory, as originally proposed by Volta, received a fatal blow when the law of conservation of energy became clearly understood, yet in its place a *new contact theory* has arisen, supported by novel and important experimental evidence, which has again been placed by recent writers on electrical science in formidable opposition to its old rival.

The old contact theory of Volta had its origin in an entire ignorance of the science of energy. It simply referred the current produced through the circuit of a pile to the effect of the metallic contacts, and it ignored the thermal and chemical changes which are also necessarily present; but it had to be finally abandoned when once it became clearly understood that the appearance of a current involved the disappearance of some other energy, actual or potential, as an invariable accompani-

ment. The new contact theory may be said to have had its source in the discovery of Sir W. Thomson, that there is undoubtedly a difference of potential produced when dissimilar metals are placed in contact—a fact not only abundantly proved by Thomson by direct experimental evidence, but, as he has pointed out, confirmed in a remarkable way by the phenomena of the Peltier effect, which, when interpreted by the dynamical theory of heat, furnish the most reliable measure of its amount. These facts, together with others presently to be referred to, have been made to furnish the key to a fresh explanation of the dynamics of the galvanic cell, which I have ventured to call the *new* contact theory, as opposed to the old or voltaic one.

It is not possible, however, to define in a few words the precise details of the new theory; they can only be arrived at by collecting together the statements as we find them laid down by their authors. The object of the present paper is to draw the attention of those interested in this question to the objections that may be raised against this new contact theory—objections based on facts, some old and some which perhaps may prove new, but all of which alike seem to throw fresh difficulties in the way of this theory, although capable of simple explanation by the old chemical hypothesis. It will be necessary then to review briefly the precise statements of this new contact theory, in order to show exactly what are the points against which objection may be taken. This will be best accomplished by collecting the statements of its principal supporters and arranging together their explanations of the phenomena which arise

(1) when dissimilar metals at the same temperature are placed in contact,

(2) when one insulated metal is placed in a liquid capable of acting chemically upon it,

(3) when two different metals are placed insulated and unconnected in one such liquid,

(4) when the two metals are joined across by a metallic arc or when two or more cells are joined up in series.

1. That the contact of metals is always attended with the production of a difference of potential between them was for a long time denied by ardent supporters of the chemical theory. De la Rive endeavoured to show that the effects observed might be attributed to oxidation; but his experiments are not conclusive;

and to Sir W. Thomson belongs the credit of having established the fact by experiment, irrespective of his theoretical deductions from the facts of thermoelectricity. He thus describes his decisive experiment:—

"A metal bar insulated so as to be movable about an axis perpendicular to the plane of a metal ring, made up half of copper and half of zinc, the two halves being soldered together, turns *from* the zinc *towards* the copper when positively electrified, and *from* the copper *towards* the zinc when negatively electrified"*. The difference of potential he finds to be about ·6 or ·7 of that of a Daniell's cell when the metals are perfectly clean; but by oxidation of the copper it may be made equal to or even greater than that of a Daniell's cell. He has also shown that if zinc and copper cylinders be connected by a wire, the electrometer detects a difference between the potentials of the air in the interior, and, lastly, that if copper filings be allowed to fall from a copper funnel in contact with a vertical zinc cylinder, they convey a negative charge to a receiver placed below. Sir W. Thomson concludes that there is sufficient evidence to show that zinc and copper attract one another chemically at any distance if connected by a fine wire, and that, as Professor Tait remarks, "when *any* two bodies of different kinds are brought into contact, there is a certain amount of exhaustion of the potential energy of chemical affinity between them, and that the equivalent of this is, partly at least, developed in the new potential form of a separation of the so-called electric fluids, one of the bodies receiving a positive, the other a negative change, the quantity depending on the nature and form of the bodies"†.

This is equivalent to saying that at the surface of contact there is perpetually a force tending to separate the two electricities in a direction perpendicular to that surface, while at all points ever so little within it there is no such force.

Professor Maxwell reiterates essentially the same facts. He gives Thomson's proof that the electromotive contact-force at a junction of two metals is represented by PJ, where P= the coefficient of the Peltier effect, or the heat absorbed at the junction due to the passage of a unit of current for a unit of time; and J is Joule's equivalent. He remarks that the electromotive force, as

* Reprint of papers on Electrostatics and Magnetism, p. 316, § 400.

† Thermodynamics, p. 62, § 107.

determined by this method experimentally, does not account for the whole electromotive force of a simple couple. This latter is in general far greater than that given by the Peltier effect for the same pair of metals. "Hence the greater part of Volta's force must be sought for, not at the junctions of the two metals, but at one or both of the surfaces which separate the metals from the air or other medium which forms the third element of the circuit"*.

Professor Jenkin, referring to these experiments of Thomson, adds that "In cases where no known chemical action occurs, as where zinc and copper touch each other, and yet difference of potential is produced, since this involves a redistribution of electricity, a small but definite consumption of energy must then occur; the source of this power cannot yet be said to be known"†.

2. It seems to be universally admitted that when an insulated metal is placed in a liquid capable of acting chemically upon it, a difference of potential is produced between the metal and the liquid, a sudden rise in potential taking place in passing from the metal-surface to the liquid in contact with it, or that the metal becomes negatively and the liquid positively electrified, metals differing in the degree of electrification they can produce with any one electrolyte.

3. But if we ask what are the conditions when two different metals are so immersed, we find the most contradictory statements given. Sir W. Thomson expresses his opinion thus in 1862:—"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's. I now think it quite certain that two metals dipped in one electrolytic liquid will (when polarization is done away with) reduce *two* dry pieces of the same metals when connected each to each by metallic arcs to the same potential"‡, which seems equivalent to saying that there is no difference of potential produced other than that due to dissimilar contact. Thus also Professor Tait:—"By interposing between two metals which have been electrified by contact a compound liquid or electrolyte, these metals are at once reduced to the same potential—a result which could not have been obtained by

* Treatise on Electricity and Magnetism, vol. i. p. 302.

† Electricity and Magnetism, p. 55.

‡ Electrostatics, p. 317, § 400.

connecting them by any metallic conductor. By the passage of the electricity a portion of the electrolyte is decomposed, and the potential energy thus developed is equal to that possessed by the electricity while separated in the metals"*.

Professor Jenkin advocates essentially the same views:— "When two dissimilar metals are plunged side by side into a liquid such as water or dilute sulphuric acid, they do not exhibit *any* sign of electrification; the three materials remain at one potential, or nearly so. If while the two dissimilar metals are in the liquid they are joined by metallic contact to terminal pieces of one and the same metal, these terminal pieces will be brought to the same difference of potentials as that which would be produced by direct contact between the dissimilar metals"†. This amounts simply to saying that, as long as no wires are attached to the plates of a single cell, there is no difference of potential; but that when wires are joined on, the observed difference of potential is due to the contact of the *wire* with that metal plate to which it is dissimilar.

Again:—"When a single metal is placed in contact with an electrolyte, a definite difference of potentials is produced between them; zinc in water becomes negative, copper in water becomes negative, but less so than zinc. If, however, the two metals are plunged *together* into water, the copper, zinc, and water forming a galvanic cell, all remain at one potential, and no charge of electricity is observed on any part of the system." "If a piece of copper be now joined to the zinc, it (the copper) will become negative, and the other copper plate positive, the difference of potentials being that due to the direct contact between the zinc and piece of copper *only*, the water having the effect of simply conducting the charge from the zinc to the copper plate and maintaining them at one potential"‡.

The foundation for these statements is found apparently in an experiment due to Sir W. Thomson. He finds that if half-disks of zinc and copper be arranged under a movable metallic needle maintained at a high positive potential, if they are connected by a wire or by contact, the needle moves in such a way as to show that the copper is negative and the zinc positive; while if they

* Thermodynamics, p. 66, § 116.

† Electricity and Magnetism, p. 22.

‡ Electricity and Magnetism, p. 44.

are separated by a slight interval and connected by a drop of water, *no* difference of potential is observed. Professor Jenkin also lays great stress on the fact that, whereas copper in contact with zinc becomes negative, in a single cell with wires attached it is the wire attached to the zinc that shows a negative potential. This he holds to be conclusive that the junction of the wire with the zinc plate is the real seat of the electrical separation; although he admits that there may be a slight difference due to the liquid, and that different liquids may augment or decrease this difference.

In another place he says:—"If the voltaic theory of the cell were absolutely correct, the electromotive force of the cell would depend wholly on the plates in the electrolyte, and not at all on the solution employed to connect them"*. But it has been found that the potential series of the metals is slightly changed by the solution employed to join the plates: in order to account for this fact it is necessary to treat the voltaic theory as incomplete. He adds, however, that the potential series of the metals for water, dilute acids, and ammonic chloride do not differ so much as to invalidate the theory, although the series for alkaline sulphides is quite different and anomalous†.

4. When the two plates in one electrolyte are joined by a wire, or when simple cells are joined up in series and the circuit closed by a wire, we find it stated that there is a constant separation of the electricities at the point of contact of different metals and a constant recombination, attended with decomposi-

* Electricity and Magnetism, p. 215.

† It may be remarked in passing, that this identity of the potential series for different acids may perhaps arise from a different cause, and not be altogether such a proof of the contact theory as Professor Jenkin concludes it is. Andrews has shown that, when *one* metal combines chemically with *different* acids, the amount of heat liberated is the same, or nearly so. Hence, if the metals be arranged in the order of their heat-producing power when combined with the same acid, that order will remain the same for most other acids. But the order is quite different when the metals are combined with sulphur or oxygen. It is true that this order is not the electrochemical one; but various causes may interfere to disturb it. At any rate it is sufficient to show that this fact of the partial identity of the potential series for different acids cannot by any means be claimed as conclusive of the contact theory. Moreover, although the *order* may be the same for the different liquids, we do not know that the *coefficients* are the same for each metal in every dilute acid.

tion, through the electrolyte. "Perhaps it is strictly accurate to say that the difference of potential is produced by the contact, and that the current which is maintained by it is produced by chemical action"*. And, lastly, that in a series of cells the electromotive force is due to the sum of the differences of potential produced by all the contacts.

The above quotations may be taken as affording the plainest notion of the new contact theory; and it will be seen that its fundamental propositions are briefly these:—

I. That two plates of different metals in one liquid are at the same potential when insulated and separated; *i. e.* there is *no* difference of potential due to chemical affinity.

II. In a cell series the gradual rise in potential, or the electromotive force, is due only to the dissimilar metallic contacts.

III. The chemical action in the battery is the result rather than the cause of the difference of potential, and is looked upon as an accompanying action rather than as the actual creator of the current, it having little or no share in the production of the difference of potential between the terminals.

These are, I venture to think, points not to be admitted as proved without further inquiry, and against which, as I shall hope to show, some grave if not insuperable objections may be urged, founded on other experimental evidence.

The first question to be settled is, then, whether in a series of cells the *whole* of the difference of potential between the terminals is due to the contacts, as above stated, or whether *any* portion is due to the tendency towards chemical combination existing between the metals and the electrolytes; and, as a consequence, whether in a single cell the plates are at the same potential or at different potentials, owing to the difference of chemical action upon them. Now I think this point will be sufficiently proved if we can establish by experiment, (i) that a battery of cells can be constructed without any dissimilar metallic contacts and with terminal plates of the same metal, and which shall yet exhibit difference of potential and continuous current; for if this is possible, it must follow that chemical affinity *alone* is capable of *creating* electromotive force as well as of maintaining a current, and that, in an ordinary cell-series, *some* part at least of the electromotive force is due to this cause,

* Electricity and Magnetism, pp. 53–55.

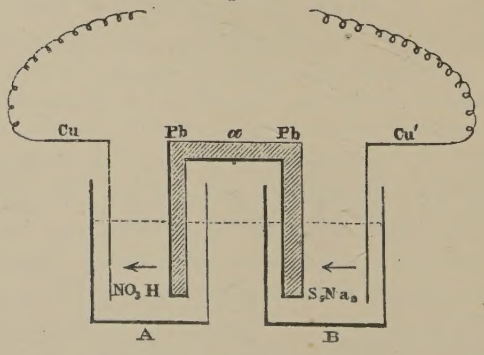
whilst the remainder is the result of the metallic contacts that may exist. Or (ii) if we can establish directly that the two plates in one cell are not at the same potential, as stated by more than one authority.

With regard to the first point, it will be remembered that an old experiment of Faraday's proved that a current can be maintained and decomposition effected by a single cell where there is no dissimilar contact. It is not easy to see how this experiment can be explained by any form of contact theory; indeed it appears unanswerable. But in order to leave no point unsettled by experiment, it seemed desirable to try and arrange a series of cells in which all dissimilar contact was absent, so that the difference of potential due to chemical action might be separated from that due to the contacts and rendered visible by the electroscope.

It is obvious that we can make no attempt to do this unless we can in some way or other obtain a battery with terminals of the same metals; for otherwise the very junctions with the electroscope introduce what we want to eliminate, viz. dissimilar metallic contact. But the following is a method by which this can be accomplished. If plates of lead and copper be placed in nitric acid the lead is positive to the copper, since it is most acted upon; but if lead and copper be placed in solutions of alkaline persulphides, then the copper is most readily acted upon and is positive to the lead; that is, the positions are reversed.

Now, if we place in a cell A dilute nitric acid and a copper

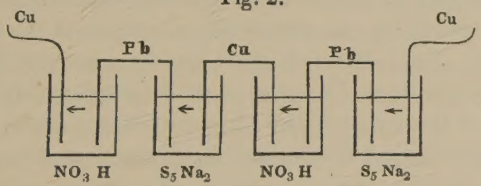
Fig. 1.



and a lead plate, we cannot join up another cell of the same sort

in series without introducing contact. But if, instead of using a cell containing acid, we place next A a cell, B, containing sodic pentasulphide, and bend over the lead plate of A to dip into the liquid in B, and place in B a so a copper plate, we shall then have two cells joined up in series without dissimilar contact and with similar metals for terminals; and yet the action of the liquids on the metals is such that in A the lead is positive to the copper Cu, and in B the copper Cu′ is positive to the lead. Hence there is a regular rise of potential in passing through the two cells; and on joining Cu Cu′ by a copper wire a current flows through both cells in the same direction, and the general effect is to urge round a current in the direction shown by the arrows. It is obvious that we need not limit ourselves to two cells. By forming a pile of alternate cells filled with acid and alkaline persulphide, connected by bent copper and lead plates alternately (fig. 2), we shall be able to accumulate differ-

Fig. 2.



ence of potential to any extent; and if the number of acid and alkaline cells be equal, we shall always end with a plate similar to that with which we began. Such a battery will exhibit a difference of potential between its two terminals when the circuit is opened, and will give a current when it is closed. In it we have nothing but chemical action to rely upon both for creating electromotive force and for maintaining the current. We have no dissimilar contacts; and as the terminal plates are similar, we can effect the junctions with the electroscope without introducing an unbalanced dissimilar contact. I have constructed such a battery of 60 cells; and by the kindness of Professor Guthrie, to whom my thanks are due, I have been permitted to compare its potential with that of a Daniell's cell, by means of a quadrant electrometer belonging to his laboratory. By this means it is at once seen that the difference of potential increases proportionally to the number of cells, the electromotive force of four cells being about equal to that of one Daniell. Joined up with a galvano-

meter it indicates a current, which, however, rapidly falls off in strength, owing to the formation of an insoluble cupric sulphide upon the copper plates. Joined up in opposition to a single Daniell's cell, with a galvanometer included in the circuit, I find that it requires from four to five cells to balance the force of the Daniell at first immersion; but after leaving it to work on short circuit for 2½ hours its electromotive force had fallen off 50 per cent.; it then required about 8 cells to bring the needle to zero. This gives for the electromotive force of two cells at first about ·5 of a volt; or the whole sixty cells are equal nearly to 15 Daniell's cells. It readily effects the decomposition of many electrolytes, and exhibits therefore every property of an ordinary cell-series. Above all, it will be noticed that since there is a regular rise in potential in passing from cell to cell, and as all parts of each plate must be at the same potential, that rise can only take place at the surfaces where the active metals are in contact with the electrolyte (that is, at the seat of the chemical action), and that therefore two metals in one electrolyte *cannot* be at exactly the same potential. But I find that more direct evidence still of this fact is to be found in an experiment of Faraday's, which seems to have escaped the notice of the contact theorists.

In his 'Experimental Researches' he gives the following fact. "I took a voltaic apparatus, consisting of a single pair of large plates, namely a cylinder of amalgamated zinc and a double cylinder of copper. These were put into a jar containing dilute sulphuric acid, and could at pleasure be placed in metallic communication by a copper wire connecting the two plates. Being thus arranged, there was no chemical action whilst the plates were not connected; on *making* the contact a spark was obtained. In this case it is evident that the first spark must have occurred before metallic contact was made, for it passed through an interval of air; and also that it must have tended to pass before the electrolytic action began, for the latter could not take place until the current passed, and the current could not pass before the spark appeared." "Hence," he says, "I think there is sufficient proof that the zinc and water were in a state of powerful *tension* previous to the actual contact"*. It is difficult to reconcile this with the experiment of the half disks and drop of water made by Sir W. Thomson. But, at any rate, a consi-

* Experimental Researches in Electricity, Series viii. ¶ 956.

deration of the whole of the facts would seem to point out that the only safe conclusion is, that in any series of cells of any sort the electromotive force is a complex effect, being due to the algebraical sum of all the differences of potential due to dissimilar contacts *plus* the algebraical sum of the differences of potential due to the chemical affinities of the metals and electrolytes *minus* any opposing force due to polarization &c.; and that so far from being the exclusive cause, the contacts can only be said strictly to have a share in producing the difference of potentials between the extremities of a battery*. And, lastly, we may with advantage compare the statements of the contact theory with certain other well-ascertained facts. Such statements, for instance, as these:—"If we close the circuit by connecting the metals by a wire, we then have constant separation of electricities at the point of contact of different metals, and constant recombination attended with decomposition through the electrolyte"†. "The electricities separated at the metallic junctions recombine through the water," "whilst the current flows the water is decomposed"‡,—which seem based on the assumption that the principal seat of the electrical actions is *not* to be looked for at the seat of the chemical actions. But, now, how does this fit in with those cases of electrochemical inversions noticed by De la Rive, where the direction of the current in a cell is *reversed* by simply diluting the electrolyte. Thus zinc is negative to tin in strong nitric acid, and mercury negative to lead; but in weak nitric acid the positions are reversed. Hence, if couples be formed of these metals in strong nitric acid, and the acid be gradually diluted, the current first ceases and then is reversed in direction.

Here, without altering the metallic junctions, we can at pleasure alter the direction of the current, and therefore also the direction of the fall in potential, since the current must flow from high to low potential. This seems conclusive that the chemical electromotive force must be even greater than the contact electromotive force. This reversal of the current, by changing the seat of the chemical activity, may be shown in another way, depending on the application of a very old principle. If

* Amounting in a Daniell's cell perhaps to 60 or 70 per cent. of the whole electromotive force.

† Tait, 'Thermodynamics,' § 116.

‡ Jenkin, 'Electricity and Magnetism,' p. 54.

plates of copper and clean iron be connected by copper wires with a galvanometer, and the iron rendered passive by immersion for a moment in strong nitric acid, then if these plates are plunged into dilute nitric acid the galvanometer indicates a strong current going through the cell from the copper to the iron. If they be removed for an instant and the iron plate touched, on again immersing the current is found to be reversed. Or we may again change the conditions, and notice that it is not sufficient to have merely two different metals and an electrolyte to form a cell. If plates of pure gold and platinum be placed in nitric acid, the most delicate galvanometer detects no current, and the same for many other pairs of metals and electrolytes.

Here we have contact of different metals producing its difference of potential; yet no current flows round "decomposing the electrolyte," as, according to the contact theory, it should do; but the instant we give play to chemical combination the ordinary results ensue. If the extremities of the copper wires from a galvanometer be attached to iron plates, and these plunged into separate cups of dilute nitric acid, on making connexion between the two cups by a bent iron plate dipping into each no current is detected. On making one limb of the connecting plate passive and re-immersing, a strong current is visible; and we find that we have the direction of the current completely under command by making any of the four plates more or less acted on than the other three.

If these experiments are to have any importance attached to them, it can scarcely be doubted that they land us in conclusions similar to the others, namely:—that we must look for the principal source of the electrical disturbance at that place where the greatest chemical activity is being brought into play; and that whereas contact of metals is in itself productive of definite electrical separation, there is in the battery another cause assisting in the production of difference of electrical potential between the terminals, viz. the potential chemical combination between the metals and electrolytes existing when the circuit is open—the energy of the current produced when the circuit is closed being, of course, the equivalent of this potential energy which disappears.

April 18, 1874.

II. *On Wind-pressure in the Human Lungs during Performance on Wind Instruments.* *By* Dr. W. H. Stone.

The object of these experiments was originally physiological. It had been stated by many writers that the forced expiration employed in playing tended to produce emphysema of the lungs; but the real amount of such pressure had never been measured.

The facts elicited had also an interest of a purely physical character, which was the principal cause of their being brought before this Society, although, the writer of the paper remarked, it was on the border-ground between two great subjects of study that new phenomena were often to be looked for.

The experiments were two in number. The first aimed simply at measuring, by means of a water-gauge, the extreme pressure which could be supported by the muscles of the lips, both in trained musicians and in persons unaccustomed to the continuous exercise of these organs. The difference between different individuals was very great, some untrained persons having naturally considerable muscular power. About 6 feet of water was the ordinary maximum when a small tube was inserted between the lips. When the lips were supported by a cupped mouthpiece, such as is used for brass instruments, a greater height of the column could be obtained. The great majority of untrained persons could not support more than three or four feet of water. It was to be noticed that the lip-muscles invariably gave way long before the expiratory power of the thoracic muscles was exhausted. By pinching the lips round the orifice of the tube with the hand, and thus preventing their yielding, a far higher column of water could be supported.

The second experiment consisted in introducing a small bent glass tube into the angle of the mouth, connected with a flexible tube passing over the shoulder. It was found that most instruments could be played as well with this addition as without it. It obviously established a communication between the cavity of the performer's mouth, and therefore of his thorax, and the pressure-gauge. The following Table was compiled from many

observations on some of our principal English musicians. The person experimented on was placed with his back to the gauge, the small tube was inserted in his mouth, and he was directed to sound in succession the chief notes of his instrument. As soon as the tone became full and steady, the position of the water-gauge was noted. A fair "mezzo-forte" note was employed. Of course, by forcing the wind and overblowing the instrument, much greater pressure could be obtained; but those given here were sufficient to produce an average orchestral tone.

Oboe . . .	lower notes	9	inches;	highest	17	inches.
Clarinet . .	„	15	„	„	8	„
Bassoon . .	„	12	„	„	24	„
Horn . . .	„	5	„	„	27	„
Cornet . . .	„	10	„	„	34	„
Trumpet . .	„	12	„	„	33	„
Euphonium .	„	3	„	„	40	„
Bombardon .	„	3	„	„	36	„

It is to be noticed that the clarinet, in this as in some other respects, differs from its kindred instruments—and also that most of the pressures are small, not exceeding or, indeed, attaining the pressure of a fit of sneezing or of coughing. They are therefore very unlikely to injure the lungs, or to produce the emphysema erroneously attributed to them.

May 9, 1874.

III. *On some Physical Properties of Ice; on the Transposition of Boulders from below to above the Ice; and on Mammoth-remains.* *By* John Rae, *M.D., LL.D., &c.*

Is the ice formed on salt water fresh? or, in other words, if ice formed on the sea is thawed, will the water obtained thereby be fresh?

For a number of years past I have spoken with many persons on the above subject; and seldom, if ever, have I found a single individual who did not say that the ice of the sea was fresh.

Some of these gentlemen are known in the scientific world; and many of them supported their opinions by quoting the highest written authorities on the subject, chiefly Tyndall's 'Forms of Water,' p. 132, par. 339, which tells us that "even when water is saturated with salt, the crystallizing force studiously rejects the salt, and devotes itself to the congelation of the water alone. *Hence the ice of sea-water, when melted, produces fresh water.*"

It is the sentence in italics to which I wish to draw particular attention.

It would be the extreme of folly and presumption on my part to question the correctness of results obtained by scientific men in their experiments in freezing small quantities of sea-water by artificial means, more especially those of the distinguished gentleman whose name I have mentioned, who, in addition to holding the high position of being one of our greatest authorities in all that relates to physical science, possesses the rare gift of being able to communicate his knowledge in such plain, clear, and forcible language, illustrated by admirable experiments, as to make his meaning fully understood, even by those who had previously been perfectly ignorant of the subject.

It is only where I have had opportunities of witnessing the action of cold carried on in a manner which may have been denied to the scientific man, that I venture to differ from him; and it is in this way that the conviction has been forced upon me, that the ice of sea-water if melted *does not* produce fresh water.

Before entering upon this subject, however, let me say a word or two on the first part of the quotation I have given.

If a saturated solution of salt is frozen, and the ice so formed is fresh, it is evident that the salt that has been "rejected" must be deposited or precipitated in a crystalline or some other solid form, because the water, if any, that remains unfrozen, being already saturated, can hold in solution no more salt than it already contains.

Could not salt be obtained readily and cheaply by this means from sea-water in cold climates?

During several long journeys on the Arctic coast, in the early spring before any thaw had taken place, the only water to be

obtained was by melting snow or ice. By experience I found that a kettleful of water could be obtained by thawing ice with a much less expenditure of fuel, and in a shorter time, than was required to obtain a similar quantity of water by thawing snow. Now, as we had to carry our fuel with us, this saving of fuel and of time was an important consideration, and we always endeavoured to get ice for this purpose. We had another inducement to test the sea-ice frequently as to its freshness or the reverse.

I presume that almost every one knows that to eat snow when it is very cold, tends to increase thirst, whereas a piece of ice in the mouth is refreshing and beneficial, however cold it may be; we were consequently always glad to get a bit of fresh ice whilst at the laborious work of hauling our heavy sledges; yet with these strong inducements we were never able to find sea-ice, *in situ**, either eatable when solid or drinkable when thawed, it being invariably much too salt. The only exception (if it may be called one) to this rule, was when we found rough ice, which, from its wasted appearance and irregular form, had evidently been the formation of a previous winter. This old ice, if projecting a foot or two above the water-level, was almost invariably fresh, and, when thawed, gave excellent drinking-water. It may be said that these pieces of fresh ice were fragments of glaciers or icebergs; but this could not be so, as they were found where neither glaciers nor icebergs are ever seen.

How is this to be accounted for? Unfortunately I have only a theory to offer in explanation.

When the sea freezes by the abstraction of heat from its surface, I do not think that the saline matter, although retained in and incorporated with the ice, assumes the solid state, unless the cold is very intense, but that it remains fluid in the form of a very strong brine enclosed in very minute cells. So long as the ice continues to float at the same level, or nearly the same level, as the sea, this brine remains; but when the ice is raised a little above the water-level, the brine, by its greater specific gravity, and probably by some solvent quality acting on the ice, gradually drains off from the ice so raised; and the small cells,

* What I mean by ice *in situ* is ice lying flat and unbroken on the sea, as formed during the winter it is formed in.

by connecting one with another downwards, become channels of drainage.

There may be several other requisites for this change of salt ice into fresh, such as temperature raised to the freezing-point, so as to enable the brine to *work out* the cell-walls into channels or tubes—that is, if my theory has any foundation in fact, which may be easily tested by any expedition passing one or more winters on the Arctic, or by any one living where ice of considerable thickness is formed on the sea, such as some parts of Norway.

All that is required, as soon as the winter has advanced far enough for the purpose, is to cut out a block of sea-ice (taking care not to be near the outflow of any fresh-water stream) about 3 feet square, remove it from the sea to some convenient position, test its saltness at the time, and at intervals repeat the testing both on its upper and lower surfaces, and observe the drainage if any.

The result of the above experiment, even if continued for a long while, *may* not be satisfactory, because the fresh ice that I have described must have been formed at least twelve months, perhaps eighteen months, before.

The Transposition of Boulders from below to above the Ice.

When boulders, small stones, sand, gravel, &c. are found lying on sea-ice, it is very generally supposed that they must have rolled down a steep place or fallen from a cliff, or been deposited by a flow of water from a river or other source. There is, however, another way in which boulders &c. get upon floe-ice, which I have not seen mentioned in any book on this subject.

During the spring of 1847, at Repulse Bay on the Arctic shores of America, I was surprised to observe, after the thaw commenced, that large boulders (some of them 3 or 4 feet in diameter) began to appear on the surface of the ice; and after a while, about the month of July, they were wholly exposed, whilst the ice below them was strong, firm, and something like 4 feet thick.

There were no cliffs or steep banks near from which these boulders could have come; and the only way in which I could

account for their appearance, was that which by subsequent observation I found to be correct.

On the shores of Repulse Bay the rise and fall of the tide are 6 or 8 feet, sometimes more. When the ice is forming in early winter, it rests, when the tide is out, on any boulders &c. that may be at or near low-water mark. At first, whilst the ice is weak, the boulders break through it; but when the ice becomes (say 2 or 3 feet) thick, it freezes firmly to the boulder, and when the tide rises, is strong enough to lift the boulder with it. Thus, once fastened to the ice, the stone continues to rise and fall with the rise and fall of each tide, until, as the winter advances, it becomes completely enclosed in the ice, which by measurement I found to attain a thickness of more than 8 feet.

Small stones, gravel, sand, and shells may be fixed in the ice in the same way.

In the spring, by the double effect of thaw and evaporation, the upper surface of the ice, to the extent of 3 feet or more, is removed, and thus the boulders, which in autumn were lying at the bottom of the sea, are now on the ice, while it is still strong and thick enough to travel with its load, before favourable winds and currents to a great distance.

The finding small stones and gravel on ice out to sea does not always prove that such ice has been near the shore at some time or other.

I have noticed that wherever the Walrus in any numbers have been for some time lying either on ice or rocks, a not inconsiderable quantity of gravel has been deposited, apparently a portion of the excreta of that animal, having probably been taken up from the bottom of the sea and swallowed along with their food.

Mammoth-remains. The position in which their Skeletons are found, &c.

In Lyell's 'Principles of Geology,' vol. i. p. 185, we read:—
"In the flat country near the mouth of the Yenesei river, Siberia, between latitudes 70° and 75° north, many skeletons of mammoths, retaining the hair and skin, have been found. The heads of most of these are said to have been turned to the south."

As far as I can find, the distinguished geologist gives no reason why the heads of the mammoths were turned to the south; nor does he say all that I think might be said of the reasons why, and the means by which the skins have been preserved for such a long period of time.

Having lived some years on the banks of two of the great rivers of America, near to where they enter Hudson's Bay, and also on the M'Kenzie, which flows into the Arctic Sea, I have had opportunities of observing what takes place on these streams, all of which have large alluvial deposits, forming flats and shallows at their mouths.

What I know to be of common occurrence in these rivers may, if we reason by analogy, have taken place in ancient times on the great rivers of Siberia, making due allowance for the much higher northern latitude to which these streams run before reaching the sea, and for the difference in size of the fauna that used to frequent their banks.

When animals, more especially those having horns, tusks, or otherwise heavily weighted heads, are drifting down a river, the position of the bodies may lie in any direction as regards the course of the stream, as long as they are in water deep enough to float them; but the moment they get into a shallow place, the head, which sinks deepest (or, as sailors say, "draws most water"), takes the ground,whilst the body, still remaining, afloat, swings to the current, just as a boat or ship does when brought to anchor in a tideway.

It is probable that the mammoths, having been drowned by breaking through the ice or in swimming across the river in spring when the banks were lined with high precipitous drifts of snow, which prevented them from getting out of the water or killed in some other way, floated down stream, perhaps for hundreds of miles, until they reached the shallows at the mouth, where the heads, loaded with a great weight of bone and tusks, would get aground in 3 or 4 feet of water, whilst the bodies still afloat would swing round with the current as already described.

The Yenesei flows from south to north, so the heads, being pointed up stream, would be to the south*.

* Not many years ago, when buffalo were very abundant on the Saskatchewan, hundreds of them were sometimes drowned in one season whilst

Supposing, then, these bodies anchored as above in 3 or 4 feet water; as soon as the winter set in, they would be frozen up in this position. The ice in so high a latitude as 70° or 75° north would acquire a thickness of 5 or 6 feet at least, so that it would freeze to the bottom on the shallows where the mammoths were anchored. In the spring, on the breaking up of the ice, this ice being solidly frozen to the muddy bottom, would not rise to the surface, but remain fixed, with its contained animal remains, and the flooded stream would rush over both, leaving a covering of mud as the water subsided.

Part of this fixed ice, but not the whole, might be thawed away during summer; and (possibly, but not necessarily) next winter a fresh layer of ice with a fresh supply of animal remains might be formed over the former stratum; and so the peculiar position and perfect state of preservation of this immense collection of extinct animals may be accounted for without having recourse to the somewhat improbable theory that a very great and sudden change had taken place in the climate of that region.

I have seen at the mouth of Hayes River in America animals frozen up as above described; but as the latitude of this place is only 57° north, the fixed ice usually wholly disappears before the next winter sets in, and liberates the animals shut up in it; but when the rivers reach the sea, as some of those of Siberia do, 1000 or 1200 miles further to the north, it may be fairly assumed that a large part of this fixed ice, protected as it would be by a layer of mud, might continue unthawed.

IV. *On the Fall in Pitch of Strained Wires through which a Galvanic Current is passing.* *By* Dr. W. H. Stone.

The object of this paper was to apply the vibrations of sound to the measurement of electrical currents, and to distinguish what was due to heating-effects from those caused by alteration of elasticity.

---

swimming across the river; and many reindeer, moose, and other animals are annually destroyed in this way in other large American rivers.

Sir Charles Lyell mentions a number of yaks being seen frozen up in one of the Siberian rivers, which, on the breaking up of the ice in spring, would be liberated and float down the stream.

Strings of brass and steel, such as are used for pianofortes (No. 16 gauge), were stretched, by means of wrest-pins, across a resonant box, over bridges surmounted by brass bearings, and tuned to unison. On passing a current from two or more Grove's batteries through them, a very marked fall in pitch was obtained. The vibrating string being 24 inches long, and tuned to two-foot C, the tone sank above a fourth in steel and a major third in brass.

This result being a compound of actual lengthening by heat and of other causes, it was, in a second experiment, endeavoured to eliminate the former element by straining similar strings between the same bridges by means of a weight. This was attached to the arm of a bent lever, to the short end of which the string was made fast. By shifting the position of a four-pound weight along the arm, very accurate unison, or definite periodicity of beats could be obtained. When the current from the battery was passed through this string, free to expand by the falling of the weight, and therefore at a constant tension, a fall of pitch was still noticed. There was also a very marked loss of tone, which, on approaching a red heat, amounted to total extinction of sound.

A third experiment exhibited the changes of electrical resistance in a wire subjected to variations of strain. The wire was accurately balanced against another resistance in a Wheatstone's bridge, and the spot of light from a mirror-galvanometer joining the two circuits thrown on the screen. On suddenly increasing the tension and raising the musical pitch of the string, the galvanometer was visibly deflected. This was not an effect of heat (since the balance had been brought about during the passage of the current), and must be due to altered molecular state caused by the strain.

It was incidentally noticed that, when beats were produced by two strings on the same sonometer, they continued to be sensible to the touch by laying the hand on the instrument long after, from diminution of amplitude in the vibration, or from slowness in the beats themselves, they had ceased to be audible. This afforded a good demonstration of the continuity of sensation in touch and hearing.

V. *On a simple Method of Illustrating the chief Phenomena of Wave-Motion by means of Flexible Cords. By the late* W. S. Davis, *F.R.A.S., Derby.*

[With a Plate.]

The simple methods about to be described, of exhibiting the chief phenomena of wave-motion, were suggested during some experiments lately made by the author on the refraction of liquid waves*. These experiments consisted in the production of waves on the surfaces of two liquids of different densities, lying side by side: on agitating the surface of either liquid, waves were produced which passed from one liquid to the other, at the same time undergoing changes in amplitude, length, and form of front. In preparing diagrams to represent these phenomena it became necessary to make drawings of vertical sections through the two liquids, perpendicular to their line of separation.

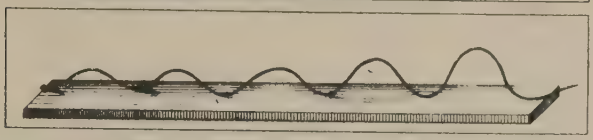
The appearance presented by the sinuous lines on these diagrams immediately suggested that a similar appearance could be exhibited by means of waves on flexible cords. India-rubber tubes, variously suspended, and both empty and loaded, were tried without satisfactory success; the waves moved too quickly to be well observed, and the reflected waves interfered with the direct ones. Further experiments led the author to devise the simple apparatus now exhibited, which, however, has been made to serve for many other illustrations of wave-motion in addition to those it was at first intended to show.

The apparatus consists essentially of:—(1) a piece of stout board about 20 feet long and 9 inches wide, which should be painted black; and (2) three or four ropes, which must be both heavy and flexible: the ropes used by builders for securing their scaffolding have been found to answer very well, especially if they have been in use some time. To enable the eye to readily distinguish any particular rope when two or more are used together, it is well to cover the ropes with differently coloured fabrics, say red, blue, and green. A few other accessories are necessary, which will be described as they are required.

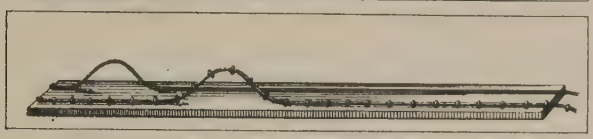
By means of this apparatus waves may be produced which

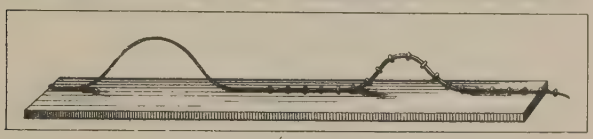
* See Brit. Assoc. Report, 1873.

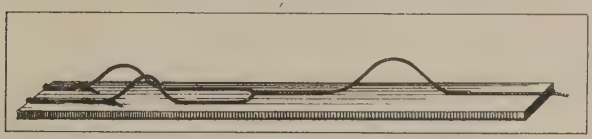
1.

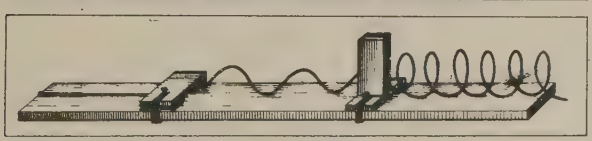

2.

3.

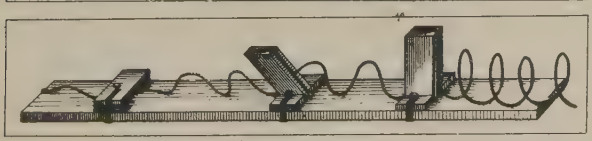

4.

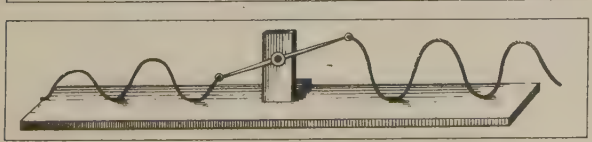

5.


6.


7.

8.


9.


10.

move slowly enough to be readily examined by the eye. The chief phenomena of wave-motion which can be shown are as follows :—

1. *Transmission of a Wave.*—One end of a rope, a few feet longer than the board, is fixed to a hook at the end of the board. The free end of the rope is then taken in the hand, and, the the rope being quite slack, a sudden up-and-down movement of the hand is made. A protuberance is thus formed which moves very slowly along the rope, presenting the appearance shown in Plate I. fig. 1*.

A single up-and-down movement produces a wave consisting of a crest only, the trough being suppressed by the board; if, however, with the rope very slack, the hand be moved up and down very quickly and energetically, a series of waves, consisting of both crest and trough, are produced (fig. 2).

2. *Amplitude and Wave-length.*—Waves having any length, from 1 to 6 or 7 feet, and amplitudes of similar dimensions, are easily produced by properly controlling the rapidity and energy of the motion of the hand.

3. *Decrease of Intensity with Distance.*—This is illustrated by a succession of waves produced by the well-timed motion of the hand (fig. 2). The actual decrease of amplitude in this case is, of course, due to the loss of energy by friction, and not to lateral spreading.

4. *Relation of Velocity to Elasticity.*—Two similar ropes, one covered with red and the other with blue, are laid side by side along the board and fastened to hooks at one end. The free ends of the ropes are held in the hand, with the finger between them, and, care being taken that they are equally loose, the hand is moved up and down as usual. The result is that a wave of the same height and length is produced on each rope, and the two waves travel side by side to the ends of the ropes. The experiment is repeated with one rope somewhat tighter than the other, when the wave on the tighter rope is observed to travel faster than that on the looser one (fig. 3). On continuing to tighten the rope the velocity of the wave is more and more increased, and may be caused to reach the end of the rope a whole length or more before its fellow.

* The *length* of the board in the figures is drawn to a much smaller scale than the other parts.

5. *Relation of Velocity to Density.*—To exhibit this relation a loaded rope is required. That now used has strung upon it a number of rings of lead cut from a leaden water-pipe; these are placed about 6 inches apart, and are covered with india-rubber bands to prevent their making unpleasant noise. The loaded and an unloaded rope are laid on the board side by side, and fixed at one end. Then, taking care the tension is equal in the two ropes, waves are simultaneously generated on them, as before described. It is then observed that the wave on the loaded rope lags considerably behind the other (fig. 4). By sufficiently tightening the loaded rope the velocity of its waves may be made equal to, or even greater than that of the waves of the unloaded rope. This may be used to explain why the velocity of sound in water is greater than in the much less dense medium, air.

6. *Transmission of Waves from one Medium to another of different Density.*—The loaded cord is attached end to end to one much lighter than itself; the united cords are laid on the board with the splice at about the middle of its length. Then, fastening the end of the lighter cord, waves are generated on the heavier one. These waves pass onwards to the lighter cord, on reaching which they acquire greater amplitude, velocity, and length (fig. 5). If the heavier cord be fixed and waves be generated on the lighter one, the reverse changes to those just stated occur on the waves reaching the heavier cord. It is an interesting experiment to transmit waves along a succession of three or more cords alternately heavy and light. With three cords joined end to end, the middle one being heavier than the others, a good illustration is produced of the changes of velocity, length, and amplitude which ætherial waves undergo in passing perpendicularly through a medium with parallel faces.

7. *Separation of a Wave into two or more smaller Waves.*—A single cord extending half the length of the board is joined to a double one extending the other half. Waves are transmitted from the single cord to the double one; on reaching the latter each wave divides in two, one wave traversing one part of the double cord, and the other wave the other part. By giving each part of the double cord a different tension, the velocity of the waves will be different in each (fig. 6). The waves on the double cord may be made to move in planes at right angles to each

other by the use of proper guides, thus furnishing an illustration of some of the phenomena of double refraction.

8. *Superposition and Interference.*—The same arrangement is used as in 7, but the waves are transmitted from the double cord to the single one. With equal tension in each part of the double cord, the waves simultaneously produced on each part run side by side until they enter the single cord, when they are superposed and produce a wave of double amplitude. One half of the double cord may be tightened until its wave reaches the single cord half a wave's length before the wave on the other half, when interference occurs, there being little or no lateral motion to be observed in the single cord.

9. *Plane of Waves.*—In the experiments previously described the waves were transmitted in a vertical plane; but by properly directing the motion of the hand, the waves may be transmitted in planes variously inclined to the board, or in a plane parallel with it. Waves in space of three dimensions, corresponding to circularly polarized light, are produced by rapidly and regularly moving the hand in a circle, the cord then taking the form shown at the right of figs. 7 and 9.

10. *Polarization.*—A series of flat boards are used as guides, which are clamped on the long board. These are shown in figs. 7, 8, 9. The vertical and oblique guides are each in two pieces, which are so approximated to each other as to just allow the cord to move freely between them. The horizontal guide is in one piece only. The vertical and horizontal guides being fixed as shown in figs. 7 and 8, waves in a vertical plane are transmitted from that end of the rope nearest the vertical guides; the waves then pass freely through the vertical guides, but are completely stopped by the horizontal one. Waves in a horizontal plane transmitted from the other end of the apparatus pass the horizontal guide, but are stopped by the vertical ones (fig. 8). Waves in an oblique plane transmitted from either end are resolved by the nearest guide into a component in its own plane and a component at right angles which is suppressed; the former passes on and is stopped by the next guide. Circularly polarized waves on reaching the guides are similarly resolved (fig. 7).

11. *Depolarization.*—A pair of oblique guides are required in addition to those described in 10. The arrangement of these is

shown in fig. 9, which needs no further explanation. The waves are supposed to proceed from right to left. With a single cord as in fig. 9, or with a partly double one as in fig. 6, an endless variation of experiments relating to polarization may be produced.

12. *Radiation and Absorption.*—A rod of iron about 2 feet in length, having an eye at the centre and at each end, is fixed by means of a screw or pin through the central eye to an upright support of wood clamped at about the middle of the board (fig. 10). The iron rod must be able to rotate freely about the pin in a vertical plane parallel to the board, but in no other plane. Attaching a cord to one end of the iron rod and continuing it to the end of the board, a series of properly timed waves are sent along it, when the rod vibrates in synchronism with the waves. If a second cord be attached to the other end of the rod and waves be transmitted as before, the vibrations of the rod set up waves in this cord which correspond in period and length to those on the first cord, thus furnishing an illustration of the reciprocity of radiation and absorption.

The author has reason to think that, as nearly all the above-described illustrations have been devised during the last twelve months, the method is capable of much further development and greater perfection.

May 23, 1874.

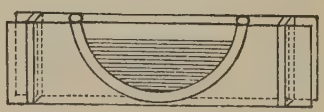
VI. *Glass Cell with Parallel Sides.*

By F. Clowes, *Esq., B.Sc., F.C.S.*

The following method has proved very convenient for making a glass cell, which may be readily fitted up from ordinary laboratory apparatus, and may also be rapidly taken to pieces for the purpose of being cleansed.

A piece of india-rubber tubing with stout walls, or, better, a length of solid rubber, is placed in the form of a letter U between two plates of glass, the ends of these plates being then firmly held together by slipping over them stout india-rubber

Fig. 1.

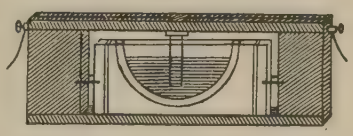


rings. A glass cell is thus obtained, the parallel faces of which are formed by the glass plates, whilst its thickness, depth, and length can be suitably varied by the stoutness and length of the rubber tube and the shape which this tube is made to assume.

With a glass cell of the size of an ordinary magic-lantern slide (fig. 1), the difference in specific gravity between hot and cold water* may be well shown upon the screen by a magic lantern, the liquid admitted by a pipette being preferably tinged by dissolving in it a crystal of potassium permanganate; and the convective currents occurring in the mass of a liquid may be thrown upon the screen by passing a galvanic current through a fine platinum wire stretched between two thick copper wires beneath the surface of the liquid in the cell: these currents are rendered much more evident by allowing the platinum wire to be immersed in a stratum of potassium-permanganate solution which has been cautiously introduced beneath the water by means of a pipette dipping to the bottom of the cell.

A smaller cell made to fit into the wooden frame of a lantern-slide (fig. 2), which has attached to it platinum wires connected by copper wires and binding-screws with a galvanic battery, serves to project electrolytic decompositions upon the screen. Perhaps the most beautiful appearance is that presented by the crystallization of the metal from a solution of lead-acetate which is undergoing electrolysis†.

Fig. 2.



In order that the cell may be water-tight, it is necessary that the india-rubber rings should exert a somewhat powerful compression; but even under favourable circumstances slight leakage is liable to occur in about half an hour after the cell has been filled; this, however, would allow ample time for the display of any of the phenomena above alluded to. Rings cut from large-sized india-rubber tubing have been found well adapted for the construction of small cells.

* See Tyndall's 'Heat, a Mode of Motion,' pp. 173 and 174.

† Mr. W. Crookes, F.R.S., suggests the electrolysis of solution of thallium sulphate as furnishing a still more beautiful example of crystallization.

VII. *On an Absolute Galvanometer.*

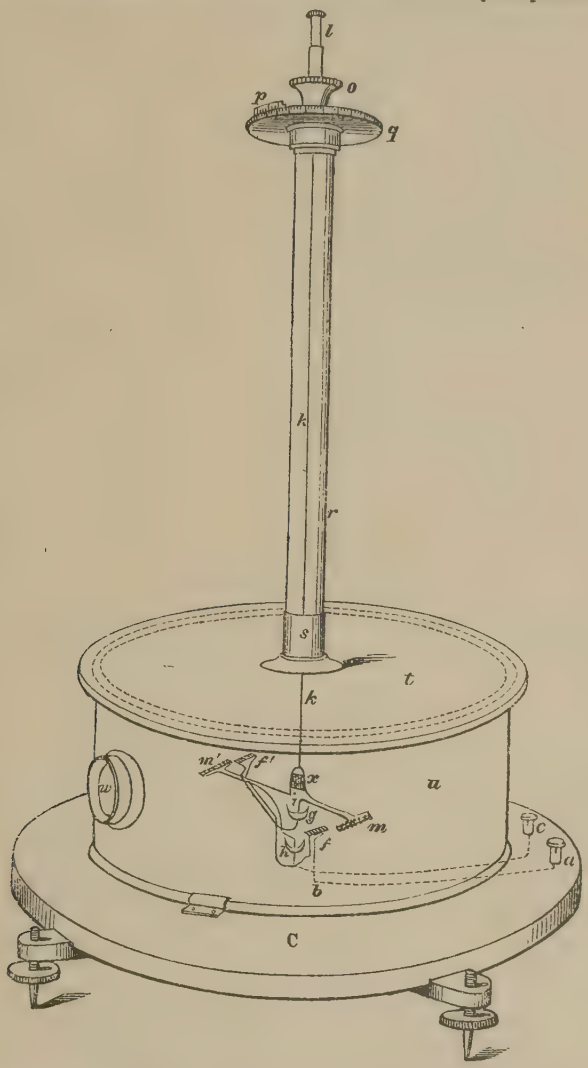
By FREDERICK GUTHRIE.

MESSRS. ELLIOTT have constructed for me a galvanometer which will, I believe, be found to possess for some purposes certain advantages over those at present in use. Its principle depends upon the measurement of the current-strength by the measurement of the mechanical force necessary to bring to a given distance of one another two electromagnets, which are excited by the current in such a fashion that they repel one another.

The current enters at *a* by the screw-clamp ; thence it passes beneath the circular wooden stand C along the copper wire *a b*. It rises vertically and coils round a soft iron mass *f*, which lies horizontal and tangential to the axis of the instrument. It passes down and across the centre of the board, then rises and coils round a soft iron mass *f'*, exactly similar and similarly placed to *f*, but on the opposite side of the instrument. Having encircled *f'*, the current-bearing wire again descends, and carries a mercury-cup *g*, through whose bottom it passes, and which is exactly in the axis of the instrument. The current then leaves the mercury by the wire *i*, which dips into it. It then traverses the wire around the iron, *m*. Thence it crosses the instrument and forms a spiral around *m'*, after which it passes into the mercury-cup *h*, and so to the binding-screw *c*. The spirals are such that there is repulsion between *f* and *m*, and also between *m'* and *f'*. It is seen that the magnetic pair *f f'* is fixed. The pair *m m'* is movable about a vertical axis. The system *m m'* is hung by a metal or glass thread *k* from the rod *l*, which works stiffly through the nut *o*. The latter carries an arm and vernier, *p*, which slides over the graduated head, *q*. The scale, nut, &c. are supported on the glass tube *r*, which is fastened by the cap *s* on to the plate-glass disk *t*, which rests upon the top of the glass cylinder *u* clamped upon the wooden base *c* resting on levelling-screws. In the side of *u* is a plate-glass window, *w*, through which a vertical line of light may be focused upon *x* (a mirror fastened to the *m m'* system), and thence thrown upon a scale in the manner which is now so often employed.

A word or two about the way in which the instrument is used. The upper plate *t* and the system *m m'* are removed by lifting *r*. The edge of *u* is rubbed with beeswax to prevent *t* from slipping

upon it. The copper wires penetrating the cups are amalgamated and a little mercury poured in. Amalgamated thin platinum-foil is then pressed into the cups, and mercury is poured upon



this. By this means a concave meniscus is obtained. The upper part is then replaced, and so adjusted by turning the plate t and the cylinder u that the mirror x is parallel to the window w, when the axis of $m m'$ makes an angle of about 15° with that

of ff'. The rod l is adjusted so that the wires of $m\,m'$ just touch the mercury; and by the levelling-screws k is so swung that m and f, and also m' and f', are exactly opposite to one another and the wires in the centres of the mercury-cups. A slit of light is then sent through w, reflected on to a screen, and the head o is then turned till the slit is split by an arbitrary vertical line on the screen. The reading of p is then noted. A current passing through the system forces $m\,m'$ away from ff'. Turn the head o until the slit of light is again brought to the mark on the screen. The angle through which it must be turned is directly proportional to the magneto-repulsion—that is, to the square of the current-strength. Many of the laws of electrodynamics may be readily illustrated by this instrument; and not only may different currents be compared with the greatest accuracy, but the absolute mechanical magneto-value of the current may be at once arrived at. By bringing the repellent magnets always to the same distance from one another, a whole class of sources of error is removed.

June 13, 1874.

VIII. *Apparatus for Measurement of Low Pressures of Gas.* By Professor M'Leod, *Indian Civil-Engineering College, Cooper's Hill.*

This apparatus was devised for estimating the pressure of a gas when its tension is so low that the indications of the barometer cannot safely be relied on, unless indeed a very wide barometer and an accurate cathetometer be employed. The method consists in condensing a known volume of the gas into a smaller space and measuring its tension under the new conditions.

The form of the apparatus is the following:—The tube a communicates with the Sprengel, and with the apparatus to be exhausted; b is a siphon-barometer with a tube about 5 millimetres in diameter; and the principal parts of the measuring-apparatus consist of c, a globe of about 48 cubic centims. capacity with the volume-tube at the top, and d the pressure-tube; these two are exactly of the same diameter, to avoid error from capillarity. The tube at the bottom of the globe is ground into a funnel-shaped portion at the top of the wide tube e; and to the side of the latter the pressure-tube d is joined. The volume-

tube at the top of the globe is graduated in millimetres from above downwards, the lowest division in this particular apparatus being 45; the pressure-tube *d* is also graduated in millimetres, the 0 being placed at the level of the 45th division on the volume-tube. A ball-and-socket joint connects the bottom of *e* with a vertical tube *f* about 800 millims. long, which is connected at its lower extremity by means of a flexible tube with the mercury-reservoir *g*; a stopcock at *h* permits the regulation of the flow of mercury into the apparatus: this may be conveniently turned by a rod, so that the operator may watch the rise of the mercury through a telescope and have the stopcock at the same time at command.

The volume-tube was calibrated in the usual way, by introducing weighed quantities of mercury into it, and making the necessary corrections for the meniscus. The capacity of the volume-tube, the globe, and upper part of the tube *e* was determined by inverting the apparatus and introducing mercury through *e* until the mercury flowed down the pressure-tube; the weight of this quantity of mercury, divided by the weight of that contained in the volume-tube, gives the ratio between the volumes; in the present case it is 1 to 54·495. While the apparatus is being exhausted, the reservoir *g* is lowered so as to prevent the mercury rising out of the tube *f*; but when it is desired to make a measurement of the pressure, the reservoir is raised and the mercury allowed to pass through the stopcock *h*. On the mercury rising into the tube *e* it cuts off the communication between the gas in the globe and that in the rest of the apparatus. Ultimately the whole of the gas in the globe is

condensed into the volume-tube; and its tension is then found by measuring the difference of level between the columns of mercury in the volume- and pressure-tubes. On dividing this difference by the ratio between the capacities of the globe and volume-tube, a number is obtained which is approximately the original pressure of the gas; this number must now be added to the difference between the columns, since it is obvious that the column in the pressure-tube is depressed by the tension of the gas in the remaining part of the apparatus; on dividing this new number once more by the ratio between the volumes the exact original tension is found.

An example will best illustrate this. A quantity of gas was compressed into the volume-tube, and the flow of mercury was arrested when its surface reached the lowermost division on the tube. The volume was then $\frac{1}{54{\cdot}495}$ of its original volume, and the difference between the levels of the mercury in the volume- and pressure-tubes was 66·9 millims.; this number, divided by 54·495, gives 1·228 as the approximate pressure. 1·2 must therefore be added to the observed column, which thus becomes 68·1; and on dividing by 54·495, the number 1·2497 is obtained as the actual pressure.

The relations existing between the contents of the other divisions of the volume-tube and the total contents of the globe were determined by measuring the tensions of the same quantity of gas when compressed into the different volumes. By this means the values of the divisions 40, 35, 30, 25, 20, 15, 10, 9, 8, 7, 6, 5, 4, 3, and 2 have been found; the experimenter is thus enabled to employ a division suitable to the quantity of gas with which he has to deal. The smallest division contains only $\frac{1}{1492{\cdot}35}$ of the globe; consequently when a quantity of gas has been condensed into this space, its original tension will be multiplied 1492·35 times. In one case an amount of gas, which originally filled the globe, exhibited a pressure of only ·5 millim. when it had been compressed into the smallest division of the volume-tube; this indicates an original pressure of only ·00033 millim.

When measuring a tension, it is advisable to make two readings under different condensations, and to take the mean of the results. The following will give some notion of the precision attainable:—

I. At division 5 ·0225
„ 2 ·0235 } Mean ·0230.

Remeasured.

At division 5 ·0228
„ 2 ·0236 } Mean ·0232.

II. Barometer 0 millim.:—

At division 10 ·1985
„ 5 ·1980 } Mean ·1982.

Remeasured.

At division 10 ·1953
„ 5 ·1967 } Mean ·1960.

III. Barometer 0·6 millim.:—

At division 15 ·5488
„ 10 ·5488
„ 6 ·5501 } Mean ·5492.

Remeasured.

At division 15 ·5464
„ 10 ·5464
„ 6 ·5480 } Mean ·5469.

IV. Barometer 1 millim.:—

At division 20 1·2042
„ 15 1·2069 } Mean 1·2055.

Remeasured.

At division 20 1·2082
„ 15 1·2099 } Mean 1·2090.

V. Barometer 1·5 millim.:—

At division 30 1·9139
„ 25 1·9080 } Mean 1·9109.

Remeasured.

At division 30 1·9041
„ 25 1·9039 } Mean 1·9040.

VI. Barometer 2·1 millims.:—

At division 35 2·6017
„ 30 2·6073 } Mean 2·6045.

Remeasured.

At division 35 2·6160
„ 30 2·6220 } Mean 2·6190.

It may be mentioned incidentally that connexions for apparatus may be conveniently made by means of ball-and-socket joints of glass. The ball is made by thickening a piece of tube in the blowpipe-flame, and the socket by cutting in half a thick bulb blown on a glass tube. The ball is then ground into the socket by means of emery and solution of soda, and afterwards polished with rouge and soda solution. When slightly greased and with a small quantity of mercury in the cup, a joint is obtained which is perfectly air-tight and flexible*.

IX. *On a simple Arrangement by which the Coloured Rings of Uniaxial and Biaxial Crystals may be shown in a common Microscope.* *By* Dr. W. H. Stone.

The author was not aware that any arrangement had been hitherto supplied to the ordinary microscope other than an extra top to the eyepiece containing a supplementary stage and an analyzer. This could only be considered a clumsy expedient.

The objects to be obtained were clearly two :—first, to transmit the rays at considerable obliquity through the plate of crystal; secondly, to gather these up and form a real image within the tube of the microscope. Amici had accomplished this by a special combination of lenses which bears his name; it might, however, be done simply by placing a screwed diaphragm on the end of the upper draw-tube within the body of the microscope. The screw should be that ordinarily used for object-glasses. To this an object-glass of long focus was fitted, and another of higher magnifying-power in the usual place. The whole body was then drawn out and adjusted to a telescopic focus on a distant object. The lower objective formed the object-glass of the telescope, and the inner objective with the Huygenian eyepiece a compound ocular. On reinserting the

* Since the above was written Dr. Sprengel has pointed out that Mr. Hartley (Proc. Roy. Soc. vol. xx. p. 141) has described as a "Sprengel joint" a connexion between two glass tubes made by grinding a conical tube into a conical cup and placing mercury or water in the cup. The difference between this and the one above mentioned is obvious: the former is quite rigid, the latter perfectly flexible.

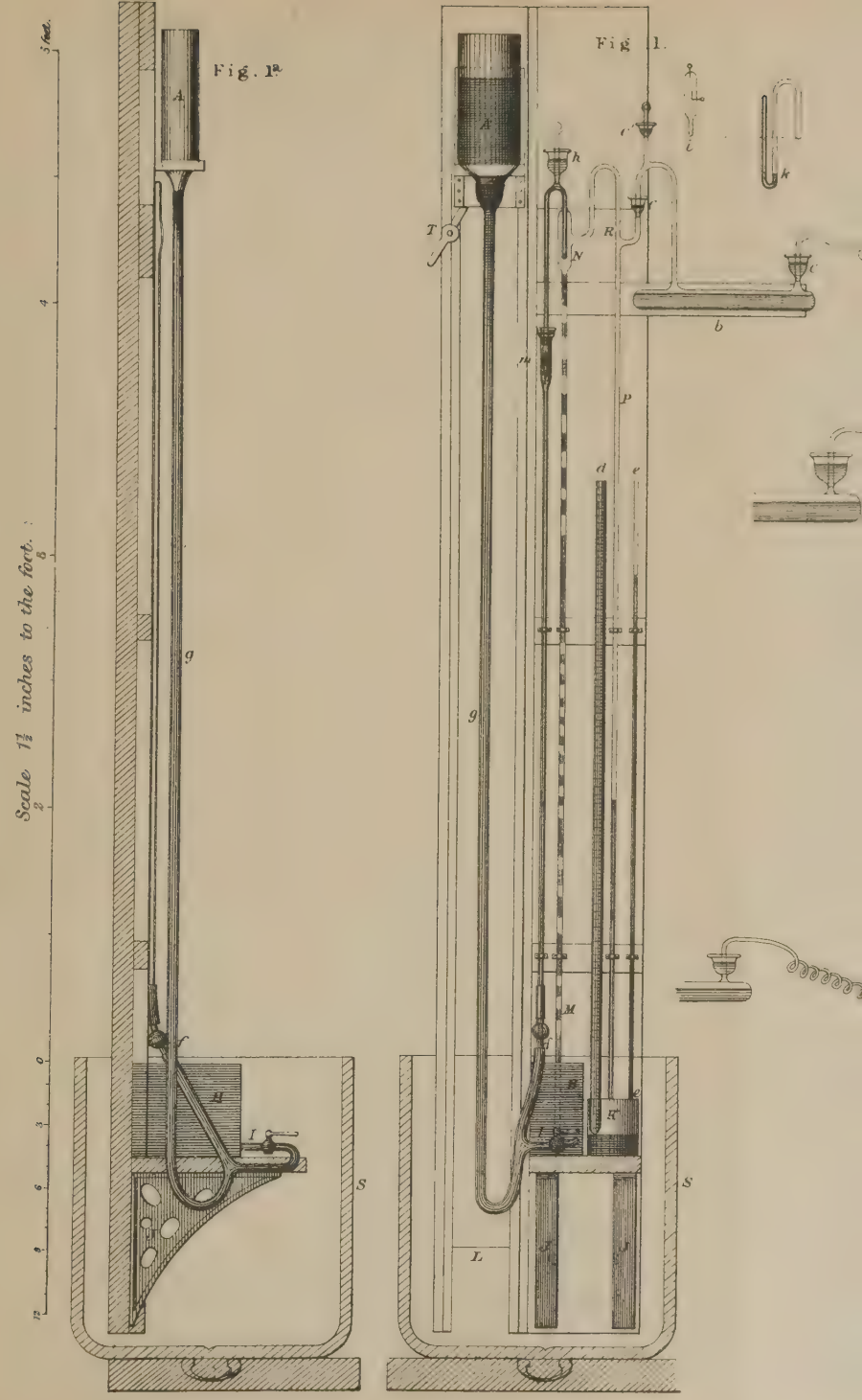


Fig. 1a
Fig. 1.
Scale 1½ inches to the foot.
A
g
H
I
S
T
N
h
R
P
d
e
M
L
b
k
l
m

Fig. 2.

Fig. 3.

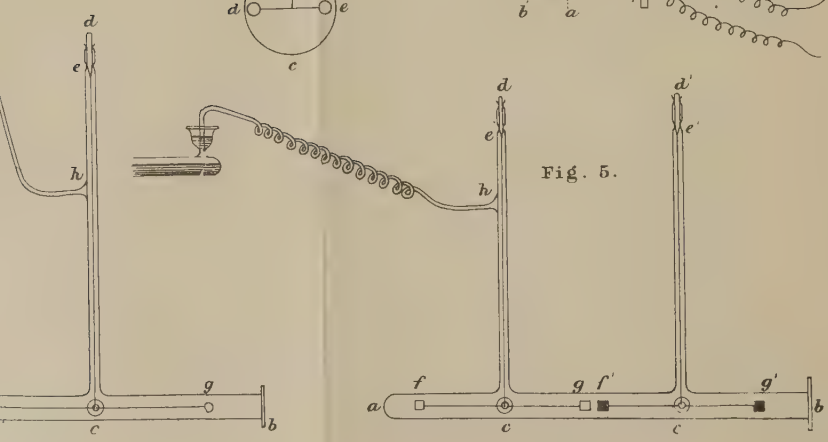


Fig. 5.

body thus arranged, and illuminating the crystal on the stage with convergent light by means of a condenser, the rings and brushes could be perfectly seen. The whole double series of rings in a biaxial crystal of carbonate of lead was thus shown.

The condenser used was a "kettle-drum" of two plano-convex lenses. The objective on the nozzle of the microscope was a $\frac{2}{3}$ of Ross; that within the draw-tube a 3-inch objective of the same maker.

June 20, 1874.

X. *On Attraction and Repulsion accompanying Radiation.* *By* William Crookes, *F.R.S. &c.*

[With a Plate.]

Before describing the apparatus and experiments which illustrate the attraction and repulsion accompanying radiation, it will perhaps be best to draw attention to the modification of the Sprengel pump which has so materially assisted me in this investigation.

Fig. 1 (Plate II.) shows the pump as now in use. Working so much with this instrument, I have endeavoured to avoid the inconveniences attending the usual mode of raising mercury from the lower to the upper reservoir. The mercury is contained in a closed glass reservoir A, perforated with a fine hole at the top. This reservoir is attached to a block capable of free movement in a vertical line and running in grooves, and connected with the lower reservoir by a flexible tube *g*. This tubing is specially made to stand a considerable pressure of mercury. It consists of a double thickness of india-rubber tubing enclosing a canvas tube in the centre, the whole being vulcanized together.

When the whole of the mercury has run through the pump, the reservoir and slide can be lowered by liberating a detent, T, and letting it descend to the block L. H is a glass reservoir which receives the mercury after flowing through the pump. When the reservoir A is emptied and has been lowered to the block L, the mercury from H is admitted into A by opening

the tap I. At f is another tap, of platinum, to regulate the flow of mercury through the pump. c, c, c' are mercury joints, it being inconvenient to have the apparatus in one piece of tubing, and not always possible to seal the different portions together by fusion. $e\,e$ is a barometer dipping into the same vessel as the gauge-barometer P, the two thus forming a differential system by which the rarity of the atmosphere in the apparatus undergoing exhaustion can be easily estimated. $d\,d$ is a millimetre-scale with pointed end, attached to the gauge and capable of being raised or lowered so as to make the point just touch the surface of the mercury. b is a reservoir of strong sulphuric acid, exposing as much surface as possible, but allowing the air to pass across it without resistance. The mercury joint c' may either be closed with a piece of glass rod ground in, or it may have either of the two pieces of apparatus i and k fitted to it. k is a mercurial siphon gauge, which is useful for measuring very high rarefactions in experiments where difference of pressure equal to a tenth of a millimetre of mercury is important. i is for still higher rarefactions; it is simply a small tube having platinum wires sealed in, and intended to be attached to an induction-coil. At exhaustions beyond the capabilities of the mercurial gauge I can still get valuable indications of the nearness to a perfect vacuum by the resistance of this tube. I have frequently carried exhaustion to such a point that an induction-spark will not strike across the small distance ($\frac{1}{4}$ inch) separating the wires of the vacuum-tube. h is the mercury-tap usually employed for letting air into the apparatus, and also for moistening the interior of the pump with oil of vitriol. l is a spiral of glass for attaching the various pieces of apparatus requiring exhaustion. As blown or fused joints are indispensable, this form of connecting piece is adopted to ensure the necessary flexibility. m is a trap to catch any air which might leak in through the platinum tap f, or the various joints in the lower part of the tubing g.

The reservoir A being filled with mercury, the tap I is turned off and the reservoir is raised to the top of the slide where it is supported by the detent T. On opening the tap f the mercury rises in the tube $f\,h$, and, falling through the chamber N, carries with it the air contained in the tube R, and in the apparatus attached to the tube l, as in the ordinary Sprengel pump. At N

the tubing is enlarged in order that the mercury may not be forced up the tube R, as otherwise frequently happens if the tubes or the mercury gets soiled.

J, J are iron brackets supporting the apparatus. S is a large inverted glass receiver, to collect the small portions of mercury which are unavoidably and constantly being spilled; it should contain a little weak alkaline solution.

The part of the tubing *g, f, h,* N forms a barometric siphon arrangement, which effectually prevents air getting into the pump from the reservoir A when the mercury has completely run out. In this case no harm whatever is done to the operation: the vacuum is not injured; and the exhaustion proceeds immediately on retransferring the mercury from the reservoir H to the reservoir A, and raising A again into its place. The apparatus, as thus arranged, is readily manageable with certainty of obtaining a barometric vacuum.

The mercury fall-tube of a pump in constant use frequently wants cleaning. I find the most effectual means of doing this is to put oil of vitriol into the funnel *h*, and then, by slightly loosening the glass stopper, allow a little of the strong acid to be carried down the tube with the mercury. With care this can be effected without interfering with the progress of exhaustion. The residual acid adhering to the walls of the chamber N does good rather than harm. When sufficient sulphuric acid has run into the fall-tube, the funnel-stopper can be perfectly closed by pressing it in with a slight twist and then filling up with mercury.

Many physicists have worked on the subject of attraction and repulsion by heat. In 1792 the Rev. A. Bennet recorded the fact that a light substance delicately suspended in air was attracted by warm bodies; this he ascribed to air-currents. When, by means of a lens, light was focused on one end of a delicately suspended arm, either in air or in an exhausted receiver, no motion could be perceived distinguishable from the effects of heat. After Mr. Bennet the subject has been more or less noticed by Laplace, Libri, Fresnel, Saigey, Forbes, Baden Powell, and Faye; but the results have been unsatisfactory and contradictory.

My first experiments were performed with apparatus made on the principle of the balance. An exceedingly fine and light arm is delicately suspended in a glass tube by a double-pointed

needle; and at the ends are affixed balls of various materials. Amongst the substances thus experimented on I may mention pith, glass, charcoal, wood, ivory, cork, selenium, platinum, silver, aluminium, magnesium, and various other metals. The beam is usually either of glass or straw.

The apparatus, consisting of a straw beam and pith-ball ends, being fitted up as here shown attached to the pump, and the whole being full of air to begin with, I pass a spirit-lamp across the upper part of the tube just over one of the pith-balls. The ball rises. The same effect is produced when a bulb of hot water or even the warm finger is placed over the pith-ball.

On working the pump and repeating the experiment, the attraction to the hot body gets less and less, until it becomes *nil*, and after a certain barometric pressure is passed, the attraction gives place to repulsion, which gets stronger and stronger as the vacuum approaches perfection.

In order to illustrate more strikingly the influence exerted by a trace of residual air, an apparatus (fig. 2) is here shown in which the source of heat (a platinum spiral, *a*, rendered incandescent by electricity) is inside the glass tube instead of outside it as before. A mass of magnesium, *b*, turned conical, is suspended in a glass tube, *c d e*, by a fine platinum wire of such a length as to vibrate seconds. The upper end of the platinum wire is sealed into the glass at *e*, and passes through to the outside for the purpose of electrical experiments. The platinum spiral is arranged so that when the pendulum hangs free the magnesium mass is about $\frac{1}{4}$ inch from it. In air the red-hot spiral produces decided attraction on the magnesium; and by properly timing the contacts with the battery, a considerable swing can be accumulated. On perfectly exhausting the apparatus, however, the incandescent spiral is found to energetically repel, and a very few contacts and breaks properly timed are sufficient to get up the full swing the pendulum is capable of.

A simpler form of the apparatus for exhibiting the phenomena of attraction in air and repulsion in a vacuum consists of a long glass tube *a b* (fig. 3) with a globe *c* at one end. A light index of glass with pith-balls at the ends *d*, *e* is suspended in this globe by means of a cocoon fibre. When the apparatus is full of air at ordinary pressure, a ray of heat or light falling on one of the pith-balls gives a movement indicating attraction.

When the apparatus is exhausted until the barometric gauge shows a depression of 12 millims. below the barometer, neither attraction nor repulsion results when radiant light or heat falls on the pith. When the vacuum is as good as the pump will produce, strong repulsion is shown when radiation is allowed to fall on one end of the index. The heat of the hand, or even of the body several feet off, is quite sufficient. The action is in proportion to the surface acted on rather than to the mass.

The barometric position of the neutral point dividing attraction from repulsion varies with the density of the mass on which radiation falls, on the ratio of its mass to its surface, and in a less degree on the intensity of radiation. In the case of pith it is seen to lie at about 12 millims. below a barometric vacuum, whilst with a heavy metal it is within a tenth of a millim. of a vacuum. Experiments to try to determine the law governing the position of the neutral point are now in progress.

Ice, or a cold substance, produces the opposite effects to heat. Thus a bar of pith suspended in a vacuum is energetically repelled by the warm hand, whilst it is as strongly attracted by a piece of ice. Cold being simply negative heat, it is not easy at first sight to understand how it can produce attraction. The law of exchanges, however, explains this perfectly. The pith index and the whole of the surrounding bodies are incessantly exchanging heat-rays; and under ordinary circumstances the income and expenditure of heat are in equilibrium. A piece of ice brought near one end of the index cuts off the influx of heat to it from that side, and therefore allows an excess of heat to fall upon it from the opposite side. Attraction by a cold body is thus seen to be only repulsion by the radiation from the opposite side of the room.

Instruments of the kind just described are perhaps the best for exhibiting large and striking movements of attraction or repulsion. Two glass globes 4 inches in diameter, fitted up with bars of pith $3\frac{1}{2} \times \frac{1}{2}$ inch, are now before you. One is full of air at ordinary pressure, whilst the other is completely exhausted. A touch with a finger on a part of the globe near one extremity of the pith will drive the bar round over 90°, in the vacuum. In air the attraction is not quite so strong.

If I place a lighted candle an inch or two from the vacuous globe, the pith bar commences to oscillate. The swing gradu-

ally increases in amplitude until one or two complete revolutions are made. The torsion of the suspending fibre here interferes, and the vibrations proceed in the opposite direction. The movement continues as long as the candle burns. This continued movement ceases if the source of radiation is removed some distance off; the pith index then sets equatorially. The cause of the continued vibration when the radiant body is at a particular distance from the pith is easy to understand on the supposition that the movement is due to the direct impact of waves on the suspended body.

For more accurate experiments I prefer making the apparatus differently. Fig. 4 represents the best form. *a b* is a glass tube, to which is fused at right angles another, narrower tube, *c d*; the vertical tube is slightly contracted at *e* so as to prevent the solid stoper *d*, which just fits the bore of the tube, from falling down. The lower end of the stopper *d e* is drawn out to a point; and to this is cemented a fine glass thread about 0·001 inch diameter, or less, according to the torsion required.

At the lower end of the glass thread an aluminium stirrup and a concave glass mirror are cemented, the stirrup being so arranged that it will hold a beam *f g* having masses of any desired material at the extremities. At *c* in the horizontal tube is a plate-glass window cemented on to the tube. At *b* is also a piece of plate glass cemented on. Exhaustion is effected through a branch tube *h* projecting from the side of the upright tube. This is sealed by fusion to the spiral tube of the pump, The stopper *d e*, and the glass plates *c* and *b*, are well fastened with a cement of resin 8 parts and bee's-wax 3 parts*.

* This is the best cement I have used for standing a vacuum: for a few hours it is perfect. But at the highest exhaustions it seems to leak in the course of a day or two. India-rubber joints are of no use in these experiments, as, when the vacuum is near upon perfect, they allow oxygenized air to pass through as readily as the pump will remove it. Whenever possible the glass tubes should be united by fusion; and where this is impracticable mercury joints should be used. The best way to make these is to have a well-made perforated conical stopper, cut from plain india-rubber, fitting into the wide funnel-tube of the joint and carrying the narrow tube. Before fitting the tubes in the india-rubber this is heated in a spirit-lamp until its surface is decomposed and very sticky; it is then fitted into its place; mercury is poured over, and oil of vitriol on the top of that. When well made, this joint seems perfect.

The advantage of a glass-thread suspension is that the beam always comes back to its original position. Before you is an instrument of this description, perfectly exhausted and fitted up with pith plates at each extremity. A ray of light from the electric lamp is thrown on to the mirror *c*, and thence reflected on to the opposite wall. The approach of a finger to either extremity of the beam causes the luminous index to travel several feet, showing repulsion. A piece of ice brought near causes the spot of light to travel as much in the opposite direction.

Here is another form of the apparatus (fig. 5). The letters and description are the same as in fig. 4, the apparatus, however, being double. The pieces *f*, *g* on the end of one beam consist of platinum-foil exposing a square centimetre of surface, whilst the extremities f', g' on the other beam consist of pith plates of the same size. It has already been explained that the neutral point of rarefaction for platinum is much higher than for pith; consequently at a pressure intermediate between these two neutral points, radiation ought to cause the platinum to be attracted and the pith to be repelled. This is seen to be the case. A wide beam of radiant heat thrown in the centre of the tube on to the plates g, f' causes g to be attracted and f' to be repelled, as shown by the light reflected from the mirrors c, c'. The atmospheric pressure in the apparatus is equal to about 40 millims. of mercury.

The position of the neutral point not only depends on the density of the body acted on by radiation, as in the above case, but also on the relation of surface to mass. Thus a square centimetre of thin platinum-foil on the extremity of the beam requires a lower exhaustion for neutrality than a thicker piece exposing the same surface. Also a flat disk of platinum has a lower neutral point than the same weight of platinum in the form of a sphere.

Intensity of radiation likewise affects the neutral point. With pith extremities a point of rarefaction can be obtained at which the warm fingers repel and incandescent platinum attracts.

During the course of this lecture I have spoken frequently of repulsion by *heat*, and have used a spirit-lamp as a source of heat to illustrate the facts described. I now wish to show that these results are not confined to the heating rays of the spectrum, but that any ray, from the ultra-red to the ultra-violet, will produce repulsion in a vacuum.

In my own laboratory I have used sunlight, and have experimented with a very pure spectrum, taking precautions to avoid any overlapping or diffusion of one part of the spectrum with another. Here I can only use the electric light, and, in order to get results visible at a distance, the spectrum cannot be very long.

The spectrum is formed with one disulphide-of-carbon prism, and is projected on to the screen by a lens. Immediately behind the screen is an exhausted bulb, having a movable index with pith terminals suspended with a cocoon fibre (fig. 3). This is delicate enough to swing over 90° with a touch of the finger, and it will even move under the influence of a ray of moonlight. I first of all arrange the spectrum so that the extreme red would fall on one pith disk were it not for the screen. On removing the screen the index immediately retreats, making nearly half a revolution.

I now replace the screen, and arrange the spectrum so that the invisible ultra-violet rays are in a position to fall on the pith disk. On removing the screen the index at once behaves as it did under the influence of the red rays, and is driven away twenty or thirty degrees. The action is not so powerful as when the other end of the spectrum is used; but this may partly, if not wholly, be accounted for by the much greater concentration of energy at the red end of the spectrum, and expansion at the violet end, when using glass or disulphide-of-carbon prisms.

I now, without disturbing the position of the spectrum, interpose in the path of the rays a cell containing a solution of iodine in disulphide of carbon, which is opaque to the luminous and ultra-violet rays, but transparent to the invisible heat-rays. Not a trace of repulsion is produced. The iodine solution is now removed and the ultra-violet rays again fall on the pith, producing strong repulsion. A thick screen of clear alum cut from one of Mr. Spence's gigantic crystals is now interposed; but no effect whatever is produced by it, the ultra-violet rays acting with unabated energy. As alum cuts off all the dark heat-rays, this experiment and the one before it prove the sufficient purity of my spectrum.

The spectrum is again turned until the dark ultra-red heating rays fall on the pith. The movement of repulsion is energetic. The iodine solution, interposed, cuts off apparently none of the

action. The alum plate cuts off a considerable amount, but by no means all. On uniting the alum and the iodine solution the whole of the spectrum is obliterated, and no action is produced, whatever be the ray which would, were it not for this double sifting, fall on the pith.

Throughout the course of these investigations, which have occupied much of my spare time for some years, I have endeavoured to keep in my mind the possible explanations which may be given of the actions observed; and I have always tried, by selecting some circumstances and excluding others, to put each hypothesis to the test of experiment.

The most obvious explanation is, that the movements are due to the currents formed in the residual gas which theoretically must be present to some extent even in those vacua which are most nearly absolute.

Another explanation is, that the movements are due to electricity developed on the moving body or on the glass apparatus by the incident radiation.

A third explanation has been put forward by Professor Osborne Reynolds, in a paper which was read before the Royal Society on June 18th last. He considers the results to be due to evaporation and condensation.

I will discuss these explanations in order.

First, the air-current theory. However strong may be the reasons in favour of this explanation, they are, I think, answered irrefragably by the phenomena themselves. It is most difficult to believe that the residual air in a Sprengel vacuum, when the gauge and barometer are level, can exert, when gently warmed by the finger, an upward force capable of instantly overcoming the inertia of a mass of matter weighing 20 or 30 grains. It must be remembered that the upward current supposed to do this is simply due to the diminished weight of a portion of the gas caused by its increase in volume by the heat applied.

An air-current produced by heat may possibly cause the beam of a balance to rise, may drive a suspended index sideways, and by a liberal assumption of eddies and reflections, may perhaps be imagined to cause these movements to take place sometimes in the opposite directions; but as rarefaction proceeds these actions must certainly get less, and they will

cease to be appreciable some time before a vacuum is attained: a point of no action or neutrality will be reached. But this neutral point should certainly be nearer to a vacuum when a light body like pith, exposing much surface, is under experiment, than when the mass acted on is heavy like brass; whereas in practice the contrary obtains. Pith ceases to move under the influence of radiation at a rarefaction of about 7 to 12 millims., whilst brass only ceases to be affected when the gauge and barometer are appreciably level.

But even could the phenomena up to the neutral point be explained by air-currents, these are manifestly powerless to act after this critical point is passed. If a current of air within 7 millims. of a vacuum cannot move a piece of pith, certainly the residual air in a Sprengel vacuum should not have more power; and *à fortiori* the residual gas in a perfect chemical vacuum cannot possibly move a mass of platinum.

It is, however, abundantly demonstrated that, in all cases after this critical point is reached, the repulsion by radiation is most apparent; it increases in energy as the vacuum approaches perfection, and attains its maximum when there is no air whatever present, or at all events not sufficient to permit the passage of an induction-spark.

I will now refer to the electrical explanation. Very early in my investigation, phenomena were noticed which caused me to think that electricity played a chief part in causing the movements. When a hot glass rod is held motionless against the side of an exhausted tube containing a pith index, repulsion takes place in a perfectly regular manner; but if the glass rod has been passed once or twice through the fingers, or is rubbed a few times sideways against the exhausted bulb, the index immediately moves about in a very irregular manner, sometimes being repelled from, and at others attracted to, the side of the glass, where it adheres until the electrical excitement subsides. Friction with the finger produces the same results; and a small spirit-flame causes similar, but much fainter, electrical effects. I soon ascertained, however, that, although electricity is capable of producing many movements similar to those caused by radiation, they are never so alike as to be mistaken. Electricity frequently interferes with, disturbs, or neutralizes the true action of radiation; but it acts in such a manner as to show that it is not

the primary cause of the movement. At the highest rarefactions, and when special precautions have been taken to avoid the presence of aqueous vapour, slight friction with the finger against the bulb, or a touch with the flame of a spirit-lamp, excites so much electrical disturbance in the pith and other indexes that accurate observations become impossible with them for several hours. I have tried many means of neutralizing the electrical disturbance; but they are only partially successful, and at the highest rarefactions interference through electrification is very troublesome.

I may draw attention to the following experiments, which are devised with the object of showing that the attractions and repulsions are not due to electricity.

In describing the pendulum apparatus (fig. 2) which I set in motion at the early part of this lecture, I explained that the mass of magnesium forming the weight was in metallic contact with the platinum wire which supported it, and that the upper end of this platinum wire was fused into the glass tube and passed through to the outside. With this apparatus I have tried a great number of experiments. I have connected the projecting end of the platinum wire with "earth," with either pole of an induction-coil the other being insulated more or less, with either pole of a voltaic battery, with a delicate electroscope; I have charged it with an electrophorus, and have submitted it to the most varied electrical conditions; and still, on allowing radiation to fall upon the suspended mass, I invariably obtain attraction when air is present, and repulsion in a vacuum. The heat has been applied both from the outside, so as to pass through the glass, and also inside by means of the ignited platinum wire; and the results have shown no difference in kind, but only in degree, under electrical excitement. I have obtained interference with the usual phenomena, but never of such a character as would lead me to imagine that the normal results were due to electricity.

It occurred to me that the repulsion might be due to a development of electricity on the inner surface of the glass bulb or tube under the influence of the radiation as it passed from the glass into the vacuum. This appears to be disproved by the fact that the results are exactly the same whether the radiation passes through the glass, or whether it is developed inside the apparatus as in the above instance.

I have produced exactly the same phenomena whether the exhausted apparatus has been standing insulated in the air, or whether it was completely immersed in water connected electrically with "earth," or surrounded with wet blotting-paper.

Here are two experiments which bear on this subject. A straw beam furnished with brass balls at each end is suspended on a double-pointed needle, and the brass balls and needle are placed in metallic connexion by means of fine platinum wire. The needle does not rest on the sides of the glass tube, but in steel cups, to which is soldered a platinum wire passing through the glass tube and connected with "earth." The tube is then exhausted, and the usual experiments are tried with hot and cold bodies, both with and without a wet blotting-paper cover. In all cases the moving beam behaves normally, being repelled by heat and attracted by cold.

An apparatus is prepared similar to that shown in fig. 4. The inside of the tube *a b* is lined with a cylinder of copper gauze, having holes cut in the centre for the passage of the supporting thread *d c* and the index ray of light falling on the mirror *c*, and holes at each end to admit of the plates *f* and *g* being experimented with. A wire attached to the copper gauze passes through a hole in the plate *b*, so as to give me electrical access to the copper gauze lining. Under the most diverse electrical conditions, whether insulated or connected with "earth," this apparatus behaves normally when exhausted.

A further reason why electricity is not the cause of the movements I have described is, that they are not only produced by heat, but also by ice and cold bodies. Moreover I shall presently show that any ray of the spectrum, besides those red and ultra-red rays which produce dilatation of mercury in a thermometer, excite an electric current between antimony and bismuth couples, and cause a sensation of warmth when falling on the skin, will produce the effect of repulsion in a vacuum. It is therefore to my mind abundantly proved that electricity, such as we at present know this force, is not a chief agent in these attractions and repulsions, however much it may sometimes interfere with and complicate the phenomena.

I will now discuss Professor Osborne Reynolds's theory, that the effects are the results of evaporation and condensation. In my exhausted tubes he assumes the presence of aqueous

vapour, and then argues as follows:—"When the radiated heat from the lamp falls on the pith, its temperature will rise, and any moisture on it will begin to evaporate and to drive the pith from the lamp. The evaporation will be greatest on that ball which is nearest to the lamp; therefore this ball will be driven away until the force on the other becomes equal, after which the balls will come to rest, unless momentum carries them further. On the other hand, when a piece of ice is brought near, the temperature of the pith will be reduced, and it will condense the vapour and be drawn towards the ice."

Professor Reynolds has tried an experiment with pith-balls attached to a light stem of glass and suspended by a silk thread in a glass flask. The exhaustion was obtained by boiling water in the flask and then corking it up and allowing it to cool. The gauge showed an exhaustion of from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch. The pith-balls behaved exactly as I have already shown they do at that degree of exhaustion, heat repelling and ice attracting. He found that the neutral point varied according to whether air was present with the aqueous vapour, or whether the vapour was pure water-gas. Professor Reynolds states:—"From these last two facts it appears as though a certain amount of moisture on the balls was necessary to render them sensitive to the heat. . . . . These experiments appear to show that evaporation from a surface is attended with a force tending to drive the surface back, and condensation with a force tending to draw the surface forward."

It does not appear that Professor Reynolds has tried more than a few experiments; and he admits that they were in reality undertaken to verify the explanation above quoted. I have worked experimentally on this subject for some years; and the last experiment recorded in my notebook is numbered 584. From the abundant data at my disposal, I can find many facts which will, I think, convince you that this hypothesis has been arrived at on insufficient evidence.

In the first place, I will show that the presence of moisture or of a condensable vapour is not necessary. Besides pith, which from its texture and lightness might be supposed to absorb and condense considerable quantities of vapour, I have used glass, mica, and various metals; and with a proper amount of exhaustion they all act in the same manner. The fact that the neutral point for platinum is close upon a vacuum, whilst that for pith

is so much lower, tends to show that the repulsion is not due to any recoil caused by condensable vapour leaving the surface under the influence of heat. Were it so, it would certainly require more vapour to be present when platinum had to be driven backwards than when pith had to be moved; but the contrary obtains in all cases. The rule seems to be, the greater the density the higher the neutral point.

I have worked with all kinds of vacua; that is to say, I have started with the apparatus filled with various vapours and gases (air, carbonic acid, water, iodine, hydrogen, &c.); and at the proper rarefaction I find no difference in the results which can be traced to the residual vapour. A hydrogen vacuum seems neither more nor less favourable to the phenomena than does a water vacuum, or an iodine vacuum.

If moisture be present to begin with, I find it necessary to allow the vapour to be absorbed by the sulphuric acid of the pump, and to continue the exhaustion, with repeated heating of the apparatus, until the aqueous vapour is removed. Then and then only do I get the best results.

When pith is employed as the index, it is necessary to have it thoroughly dried over sulphuric acid before using it, and during the exhaustion to keep it constantly heated to a little below its charring-point, in order to get the greatest sensitiveness.

Professor Reynolds says, "In order that these results might be obtained, it was necessary that the vapour should be free from air." On the contrary, I find the results take place with the greatest sharpness and rapidity if the residual gas consists of nothing but air or hydrogen.

Professor Reynolds further says, "Mr. Crookes only obtained his results when his vacuum was nearly as perfect as the Sprengel pump would make it. Up to this point he had nothing but the inverse effects, viz. attraction with heat and repulsion with cold." In the abstract of my paper published in the Proceedings of the Royal Society, I describe an experiment with a pith-ball apparatus in which the neutral point is 7 millims. (about $\frac{1}{4}$ inch) below the vacuum, repulsion by heat taking place at higher exhaustions. At the Royal Society *Soirée*, April 22, 1874, I showed, and fully described in print, the apparatus now before you, consisting of a pith bar suspended by a cocoon fibre in a glass bulb, from which the air is exhausted until the baro-

metric gauge shows a depression of 12 millims. below the barometer. Neither attraction nor repulsion results when radiant light or heat falls on the pith. Exhaustions of 7 and 12 millims. are certainly very inferior vacua for a Sprengel pump.

As a matter of fact, however, I have obtained repulsion by radiation at far higher pressures than these. The true effect of radiation appears to be one of repulsion at any pressure, overbalanced when a gas is present by some cause—possibly air-currents, but probably not. I have already explained that the barometric height of this neutral point dividing attraction from repulsion varies with the density of the substance on which radiation falls, on the relation which the mass bears to the surface, and on the intensity of radiation. By modifying the conditions it is not difficult to get repulsion by radiation when the apparatus is full of air at nearly the normal pressure of the atmosphere.

Professor Reynolds again says, "The reason why Mr. Crookes did not obtain the same results with a less perfect vacuum was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour." On this I may remark that the writer, before he explained how it was I could *not* get certain results, should have made sure that what he assumed to be the case was really so. I have not the least difficulty in showing repulsion by heat in imperfect vacua when mixed vapours and gases are present.

In my arguments against the air-current theory, I have shown that the best results are obtained when the vacuum is so nearly perfect that an induction-spark will not pass through it. This is an equally good argument against the presence of a condensable vapour as it is against that of air.

From the construction of my Sprengel pump I am satisfied that the vapour of mercury is absent from the apparatus.

The following experiments have been specially tried with the object of testing this theory. A tolerably thick and strong bulb is blown at the end of a piece of combustion-tubing; and in it is supported a bar of aluminium at the end of a long platinum wire. The whole is attached to the Sprengel pump, and exhaustion is kept going on for about two days, until a spark will not pass through the vacuum. During this time the bulb and its contents are frequently raised to an incipient red heat. At

the end of that time the tube is sealed off, and the bar of aluminium is found to behave exactly as it would in a less perfectly exhausted apparatus; viz. it is repelled by heat. A similar experiment, attended with similar results, has been tried with a glass index. It is impossible to conceive that in these experiments sufficient condensable gas was present to produce the effects Professor Reynolds ascribes to it. After the repeated heatings to redness at the highest attainable exhaustion (the gauge and the barometer being level for nearly the whole of the 48 hours), it is impossible that sufficient vapour or gas should condense on the movable index to be instantly driven off, by the warmth of the finger, with recoil enough to drive backwards a heavy piece of metal.

My own impression is that the repulsion accompanying radiation is directly due to the impact of the waves upon the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation. Whether the ætherial waves actually strike the substance moved, or whether at that mysterious boundary-surface separating solid from gaseous matter there are intermediary layers of condensed gas which, taking up the blow, pass it on to the layer beneath, are problems the solution of which must be left to further research.

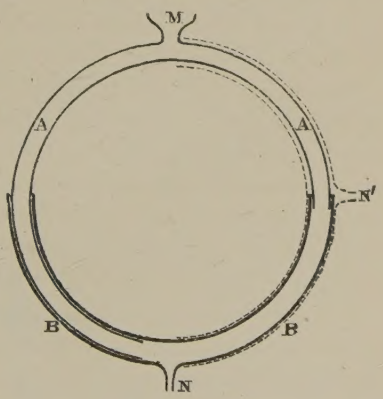
In giving what I conceive to be reasonable arguments against the three theories which have been supposed to explain these repulsions, I do not wish to insist upon any theory of my own to take their place. The one I advance is to my mind the most reasonable, and as such is useful as a working hypothesis, if the mind must have a theory to rest upon. *Any* theory will account for *some* facts; but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of either of the theories I have already discussed.

My object at present is to ascertain facts, varying the conditions of each experiment so as to find out what are the necessary and what the accidental accompaniments of the phenomena. By working steadily in this manner, letting each group of experiments point out the direction for the next group, and following up as closely as possible, not only the main line of research, but also the little bylanes which often lead to the most valuable results, after a time the facts will group themselves together

and tell their own tale. The conditions under which the phenomena invariably occur will give the laws; and the theory will follow without much difficulty. To use the eloquent language of Sir Humphry Davy, "When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them all together. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

XI. *Modification of the usual Trombone Apparatus for showing the Interference of Sound-bearing Waves. By* W. F. Barrett, *F.R.S.E. &c., Professor of Physics in the Royal College of Science, Dublin.*

A simple apparatus for showing the interference of sound-bearing waves may be made by employing a circular arrangement of tubes, one sliding within the other. One tube, A, to which the mouthpiece M is fixed, is three fourths of a circle; the other tube, B, to which the nozzle N is attached, is half a circle, and of such diameter that it slides freely over the tube A.



When the nozzle is diametrically opposite the mouthpiece, the path of the sound-waves is of equal length, and hence the sound from any convenient source placed near to or within the mouthpiece is distinctly heard. By turning the nozzle towards N', in the direction shown by the dotted lines, one limb of the tube is lengthened whilst the other is correspondingly shortened; the path of the waves being now unequal, a point is soon reached where the sound is nearly obliterated.

Employing a suitable source of sound, and a sensitive flame or a resonant jar as a phonoscope, an audience can perceive at

once the gradual destruction of the sonorous pulses; and moreover the relative lengths of the two branches of the tube clearly indicate the principle of interference thus illustrated.

One instrument I made was 2 feet in diameter, of 1-inch-square zinc tubing; another and better instrument (skilfully made by Mr. R. H. Ridout) was of brass tubing, 1 foot in diameter, the one limb being $\frac{1}{2}$-inch, the other $\frac{5}{8}$-inch tube. About 18 inches in diameter would probably be the best and most convenient size. In making the experiment, care should be taken to avoid (α) the conduction of sound to the ear by the metal substance of the instrument; (β) the direct transmission of sound through the surrounding air. The latter can be overcome by attaching a sufficiently long gutta-percha tube to M, thus removing the mouthpiece to a distance from the ear. The former can be obviated to some extent by having an inelastic mouthpiece or similar covering to the end of the tube. But Mr. Woodward's device of putting a source of sound, such as a reed, entirely within the tube, and a trumpet mouthpiece at N, is undoubtedly the best and most suitable class method of making the experiment.

P.S.—With an ordinary pitch-pipe inserted at N, I have to-day (July 25) repeated the experiment to the class of science teachers now at South Kensington. A continuous blast of air was driven through the pipe from an acoustic bellows; and the loud note heard at first was *utterly extinguished* by altering the relative lengths of the tubes. By pushing the tube still further round the note again came out; thus the sound of the pitch-pipe could be turned on and off at pleasure. Extinction is not confined to a mere line in adjusting the pipe, but spreads over a short and definite range. In this case it is probably, as Professor Goodeve suggests, the interference of two resonant columns of air, rather than the coalescence of two progressive waves in opposite phases.